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Colloquium Paper

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An ancient revisits cosmology

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ABSTRACT **In this after-dinner speech, a somewhat lighthearted attempt is made to view the observational side of physical cosmology as a subdiscipline of astrophysics, still in an early stage of sophistication and in need of more theoretical understanding. The theoretical side of cosmology, in contrast, has its deep base in general relativity. A major result of observational cosmology is that an expansion of the Universe arose from a singularity some 15 billion years ago. This has had an enormous impact on the public's view of both astronomy and theology. It places on cosmologists an extra responsibility for clear thinking and interpretation. Recently, gravitational physics caused another crisis from an unexpected observational result that nonbaryonic matter appears to dominate. Will obtaining information about this massive nonbaryonic component require that astronomers cease to rely on measurement of photons? But 40 years ago after radio astronomical techniques uncovered the high-energy universe, we happily introduced new subfields, with techniques from physics and engineering still tied to photon detection. Another historical example shows how a subfield of cosmology, big bang nucleosynthesis, grew in complexity from its spectroscopic astrophysics beginning 40 years ago. Determination of primordial abundances of lighter nuclei does illuminate conditions in the Big Bang, but the observational results faced and overcame many hurdles on the way.**

Some History, Metaphors for the Beginning, Reductionism

To justify my speaking here, I must turn time backwards and invoke authority. I have worked on and been mostly interested in how stars evolve. I am fascinated by atomic physics, the spectra of atoms, how many atoms there are of each element, and, why based on low-energy nuclear physics, there are that many. I recognize and admire some stars as individuals and enjoy the strange things they do. My last contribution to cosmology was to participate in recognizing the redshift of quasars (then called QSRS) the day Schmidt broke that logjam. Especially so since I had already developed an exotic but incorrect hypothesis, which claimed that they were stripped supernova cores. Their spectra were of supposedly unrecognizable, highly ionized extinct radioactivities. This was an incorrect but fashionable idea. Schmidt and I wrote what I still think is a good paper (1) in 1964. It provided a first model of the broad-line emitting regions of quasars; we confessed to having no explanation for the enormous energy released within so small a volume. Stars are still my passion. But at this meeting I cannot help being entranced by the growth in the kinds and amounts of data about the larger universe and by its increasing complexity. I am sure that many of you begin to realize the extent of your task; it is exciting for me to be near discoverers of the mostly unexpected. A wide gap between observation and theory must always exist. That is not a discouraging fact, and astronomers have long benefited from the contradictions they uncovered. An ideal recent example is the Cosmic Background Explorer, results from which assert that smoothness dominates at large redshift, z. But we obviously live among clumpy galaxies and clusters. Important theoretical questions arise from this contrast, which may lead to a deep insight into particle physics, as discussed extensively at this meeting.

In spite of new knowledge, one must still worry about the large gaps between "fact," "interpretation," and "meaning." I may be telling you what you already know, but, viewed from the outside, the present situation in cosmology somewhat resembles that in stellar spectroscopic astrophysics 30–40 years ago. Enormous increases have since occurred in the (*i*) available data, its quality, and wavelength range; (*ii*) laboratory astrophysics and computation of atomic structure; and (*iii*) stellar atmosphere theory, mainly from the increased power of computers. As a result, the once negative connotation of the phrase "astrophysical accuracy" has, I hope, disappeared. Some believe we know the helium/hydrogen ratio to 1% of itself. In observational cosmology, however, there is much further still to go; the expansion rate (Hubble constant), H_0, is a subject of argument at the 50% level. A star is essentially an individual, may lose or accrete matter, have its individual peculiar history, but long remain a stable, nearly closed system. Meaningful physical parameters can be assigned to it, the luminosity predicted as a function of mass, composition, and age. Stellar astronomy smells like physics. My hope is that observational cosmology may soon acquire a similar depth of physical understanding.

Especially important are accurate distances and luminosities measurable for many stars. But by its nature, a galaxy is a less rigorously closed system; gravitational and thermodynamic changes occur; it may even cannibalize neighbors. If so, it enters a stage of rapid star formation and enormous brightness. Its luminosity as a function of time is normally the sum over myriads of histories of its stellar (and interstellar gas) contents. Galaxy parallaxes cannot be measured, reflecting the oldest problem of stellar astronomers—the distance scale. If galaxies contained sufficiently luminous individual stars or groups to serve as standard candles, the galaxy distance scale could be reducible to that of the nearby stars. But at interesting galaxy distances only the most luminous supernovae are useful individuals. Why should nature assign unique properties to something as violent as gravitational collapse and the ultimate explosion producing iron-group elements? It would be puzzling if it does, in spite of remarkable successes of theory for supernova 1987a. Can a galaxy itself serve as a standard candle? What dictates the brightness distribution of its galactic clusters, old or new? Some useful, if unexplained, regularities permit observers to recognize various types of galaxies. Regularities exist that provide estimates of luminosity from such an apparently adventitious property as the internal velocity dispersion. If a physical

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theory of galaxy formation becomes available, perhaps some day one might predict a galaxy's luminosity from other observed properties. Galaxies do cover an enormous range of mass, and probably have a wide range of baryonic mass/light ratio. Perhaps external criteria will be found making it possible to estimate true luminosity. In Hubble's time, it was reasonable to talk of a mean absolute magnitude with a modest dispersion. But for 70 years even stellar astronomers worried about the Malmquist corrections, which are much worse in extragalactic studies. Everyone is conscious of the bias in any sample attainable with limited means, but some of these are subtle.

In stellar astronomy we begin to hope that theory is soundly based on known physical laws. One can even view stellar astrophysics as a slightly impoverished, even messy, part of atomic, nuclear, thermodynamic, and plasma physics. It tries to think like physics. But it may well be long before current concerns about distance scale and statistical bias are satisfied, as the physics background of observational cosmology is better understood. There is no reason for discouragement; the most encouraging change in astronomical sociology is how many scientists at this meeting have background training and experimental skills in what was called "physics." Interdisciplinary walls have largely disappeared, and cosmology offers an ultimate challenge. You must and will succeed.

Complexity exists. For a Californian at a Colloquium held near the beaches of the Pacific, images connected with water are natural. Water has an impressionistic quality. In swimming pools, light shimmers on and through water—refracted through ripples to the bottom. Hypnotized by myriads of running cusps of light, is it enough to say that they move at random? On the Pacific beach, your view of a breaking wave differs from a surfer's who uses that wave. Adjacent cells of water are irrevocably torn apart; some run up the beach, others return to the sea; their neighborhood or topology is torn. Are details predictable or meaningful? I once argued with Dick Feynman about whether we should travel to Mars; he asked if I wasn't curious about how the surf looked at the edge of a Martian ocean. I could tell him that Mars had no ocean but he had asked the deeper question. How do waves in another fluid, under a different atmosphere, depend on gravity? Outcomes of simple laws are sensitive to initial conditions and how far away is chaos? How far is it safe to push "reductionism?"

We abstract by reducing a full description of an event to the measurable part of its significant content. Can we neglect the color of a body falling under gravity? If so, how much more about it can we omit? Mechanists claim that human consciousness is "nothing but" an organized set of chemical reactions, obeying a set of computing instructions more complex than any supercomputer has yet achieved, but not different. Reductionism is quite easy. It works, since it is how politicians get elected. Reality forces gross reductionism in limiting our response to what is or may be observable.

Let us consider the Big Bang. A Greek epigram I remember partially is that "In the beginning was the word, and the word was made flesh." If that is not an exact quotation, it is a common Platonic image. First came the logos. We can paste together a mystic version from the New Testament, John 1:1 and 1:14, to get "In the beginning was the Word, and the Word was with God . . . . And the Word became flesh." The 1961 Alternate Oxford Edition is less elegant but reads "When all things began, the Word already was." For those who do not read that far in the Bible, Genesis 1:1 is quite explicit "In the beginning God created the heavens and the earth." Apparently, we astronomers had an instrument platform already on the first day, and Genesis could justify support of planetary missions and submillimeter astronomy. For speculative philosophers and for the public, the numerical values of the Hubble parameters H_0 and t_0 are seriously interesting. In current myths about cosmology, as they are publicized, there is a singularity (or perhaps divine intervention) at t_0. This fascinates the public and produces money-making books in which many of you appear as saints or devils. Theoretical cosmologists prophesy, saying: *It must be*. Fortunately for an 82-year-old, the Bible provides apt quotations. Ecclesiastes, written by and for old folks, says "Better is the end of a thing than the beginning thereof." The singularity must take care of itself, the easier task being to describe what the universe now contains. Then work backwards to as close to t_0 as possible. The cosmic background radiation and its interpretation were recent great successes in this quest. They picture a smooth happening, quite unlike the visible stars, clusters, galaxies, clusters of galaxies, walls, and voids. Such observed structures are complicated and have the particularity of any event, not all of which may be rationally understandable. We omit what makes the particularity of reality interesting, an unfortunate choice. *Most of the fun is in the details*. A dimly visible universe, in which observable baryons represent only a fraction of the total mass, is a sad fate for a cosmological enterprise that began on a small optimistic scale 70 years ago.

Most difficult for me, we may near the end of the historic dependence of astronomy on the measurement of radiation. If the theorists must have $\Omega \equiv 1.000$, if the gravitation (as beautifully observed in galaxy rotation) requires that dark matter outweigh luminous matter, our old world of observation appears dark indeed. Velocity dispersions are observed to be larger in galaxy clusters, still larger in the Hubble flow. Good and evil, locked in permanent struggle, are imaginatively represented as good is light, white (like a cowboy's hat) and evil is dark, black. Added to the peculiar velocities of galaxies are gradients in the potential produced by invisible mass. Galaxy positions and distances are useful merely to label coordinates in this potential. But what happens to the unfortunate big-telescope observers? Dark matter, if it exists, becomes the game for experimental and particle physicists. There are not enough brown dwarfs to close the Universe. It would be a sad fate for a brilliant science that for 2000 years triumphantly extracted information from very few photons, especially so when technology provides such wonderful new gadgets, and computers make thinking easy. Is it time to retire or say: *Back to the drawing board*? I hope nobody here would think so—instead let us look for an exotic particle whose decay will be a photon we could measure. Let me hope that detection will be by someone calling themself astronomer. Astronomers have remained resilient; when earlier challenges (like cosmic static) occurred, they successfully acquired the required new techniques, co-opted physicists or engineers with different skills as colleagues. The next new slogan must be: *Once more unto the breach, dear friends*. Who leads that charge?

Experiment and Theory, Some More History

I have long been connected with use or planning of big optical (and of some radio) telescopes. In the 1960s and 70s, our community grew to feel that a nearly constitutional right existed providing tools for observing fainter objects at a variety of wavelengths. All wavelength ranges are nearly covered, with planning now of increased collecting area and resolution at each wavelength. A number of experiments in space (which are very expensive) and numerous large optical telescopes (pretty expensive) are planned. They include: two Keck 10 m under construction; four ESO VLT of 8 m; two Gemini at 8 m; from Japan a most expensive 8 m; the MMT conversion, 6.5 m; the Columbus, Magellan, and other projects. This enormous increase of collecting area and capital costs would never have occurred if it had not been for the cosmological problem. Justifying such expenditures is a problem cosmologists must now face, since they bear the guilt. One or two 8-m telescopes would have kept stellar astronomers busy forever but probably would never have been funded.

But in cosmological research, do theory and telescope observation talk about the same universe? Can we honestly say that the simple data obtainable will be either (*i*) crucial, (*ii*) important, or (*iii*) mostly irrelevant to theory? Here is the point where sound theoretical understanding of cosmological observables becomes most critical. We must understand, believe, and be ready to justify our answer; credibility with the public, federal agencies, and colleagues in other subdisciplines is at stake.

But worse than scientific squabbles the supercollider caused is the current political pressure that science concentrate on improving the competitive status of U.S. industry, provide employment for the uneducated, but maintain regional balance in funding. Leaders of government, federal agencies, and college presidents repeat such nonsense, disastrous for creativity and planning. To astronomy's great advantage, we still retain public interest and support. The grandeur and beauty of the contents of our universe do capture the imagination of people. We deserve no credit for that, but we do invent imaginative names and explanations that become new poetic public metaphors. This innocent respect is deepened by the almost Freudian public emphasis on "beginnings" and the temptation to credit cosmology with substantial insight into the Beginning. Public images of cosmology and of the moment of human conception are similar. The price of public enthusiasm is that we must be honest about what we claim to know or plan to do.

Trite promises and trite phrases about funding become ritualistic; as a government advisor I often felt ill at such loose talk. One cannot justify exorbitant expenditure by claims such as: (*i*) we will lose our position of leadership, or (*ii*) if we don't spend this money, it will be wasted on an aircraft carrier, or (*iii*) the most important results will be the unpredicted ones. Since we still hear these phrases they may in part be true, but rigorous honesty is preferable. The last claim is most damning; if theory predicts so little, why test it?

We have built on the shoulders of giants, we live in a fortunate country, and we have been extremely lucky. Promise what is honestly possible; don't insist you deserve it.

Our luck consisted in the fact that as the universe was being unveiled it contained enough regularity to reveal general patterns and enough oddity that one could not remain comfortable generalizing from the little we knew. Observational cosmology has an American flavor, being semiempirical, relying on large equipment and good climate, confident that solutions would arise to explain the novelty that would certainly turn up. The cosmological search, largely American, became linked with the public respect for Einstein and his general relativity. Hubble's first major and serious paper (2) on the observed expansion is fun to read. Its diagram shows a nearly linear velocity–distance relation for 24 nebulae reaching out to 2 megaparsecs (Mpc), and to 1000 km·sec^{-1}·Mpc^{-1} (old distance scale). In spite of large scatter, the points do show an upward trend. Hubble quotes a Mount Wilson velocity of 3779 km·sec^{-1} for NGC 7619, for which his extrapolated relation gave a distance of 7 Mpc. His rate, 513 ± 60 km·sec^{-1}·Mpc^{-1}, had enormous consequences. His discussion is modest and brief. " . . . possibility that the velocity–distance relation may represent the de Sitter effect, and hence that numerical data may be introduced into discussions of the general curvature of space . . . and a general tendency of material particles to scatter. The latter involves an acceleration and hence introduces the element of time . . ." This time, while much too short ($t_0 \approx 2 \times 10^9$ yr) was still a physically interesting number. As data increased, other measurable parameters were introduced (e.g., the number counts and angular diameter vs. brightness), critical tests were devised for general relativity by R. C. Tolman and later by H. P. Robertson. These tests had, and still have, a hard time. But where would astronomy be without the crutch to imagination provided by the Hubble time? How many telescopes were and will be built and used to aim at measuring this slippery number? Granted that the time scale was wrong, the large distances and times captured the attention of all scientists, changed physicists into astronomers, endeared astronomy to the newspapers. We were always good for a new story about the new largest redshift; observational cosmologists became lords of the universe and possessors of the best observing time with large telescopes.

Radio telescopes revealed the new high-energy universe, introducing new areas of physics, and provided the very luminous radio galaxies as sources of new larger redshifts. The redshift of 3C295 at z = 0.46 more than doubled the largest z obtained after 30 years of large telescope observation by Hubble, Humason, Minkowski, and Mayall. Clearly relativity was one of the dominant great ideas of the early 20th century. But although I admired Humason and Minkowski, I confess to relief at their retirement, which made available observing time for me at the prime-focus spectrograph of the Palomar 5-m reflector. But what did I, in fact, observe? White dwarfs, at a distance of only 30 pc (not Mpc)—less dramatic objects. But they provided tests for equivalence and special relativity, from the gravitational redshift and other laws of physics. For example, their maximum mass lies below the theoretical upper limit, 1.4 Suns; not only does electron mass increase with velocity, but the hydrogen/helium ratio is low; hydrogen has burnt completely. Like the observational cosmologists, I was seduced by the interplay of relativity, stellar structure, evolution. Unexpectedly rich details did make my choice worthwhile; there was slow rotation (caused by loss of mass and angular momentum), loss of magnetic-field energy, compositions resulting from α-particle burning, and nearly complete gravitational separation of elements by diffusion. It is not cosmology but it is fun and even important.

Nucleosynthesis in the Early Universe

A story of how one reacts to a major change in point of view, to a "change in paradigm," may illuminate how observers face new challenges. The abundances of the lighter nuclei and their isotopes are now useful to Big Bang cosmology. With many collaborators, data on stellar compositions were provided, from 1950 to 1970, with eyes on the theory of stellar nucleosynthesis. Involved from the beginning, I give a largely personal account of how abundances of the lighter elements were attacked. In "Gedanken Astrophysics" (read in preprint), Kurucz (3) discusses the primordial abundances of ^{1}H, ^{2}H, ^{3,4}He, ^{6,7}Li as constraints on the early universe. I condense his words drastically: "The logical way to proceed would be to make a grid of model universes, varying the baryon and radiation densities . . . until they produce predictions that can be compared directly to observation. Then make the necessary observations and test the theories." He continues . . . "That is not the way many astrophysicists proceed, however, . . . they take observations that can be made . . . and try to determine the primordial abundances, knowing *a priori* what answer is needed to fit some particular model . . ." I must apologize to Kurucz, an important astrophysicist working on stellar model atmospheres. It certainly does not work that way. We observe only what we can; we are always limited in experiments we can perform, living in a brutally real universe, without the luxury of changing it, lacking observational tools to do what we want. Not all experimenter pioneers are also capable of developing a theory of the 3 K radiation background; their data forced the interpretation; the early prediction by Gamow *et al.* was wrong.

Cosmic abundances (more difficult than those at stellar surfaces) present nontrivial problems of interpretation. Why were primordial abundances of the relevant light nuclei not easily found? To test the theory of the early universe, we should know how much that predicted output was processed and altered through stellar interiors. One may feel uncomfortable if the $^2H/H$ ratio is determined from local interstellar matter while the H/He ratio comes from gas-starved metal-poor galaxies. The lithium, as a Li/Fe ratio in the oldest stars, remains primordial only over that limited range of surface temperatures in which it is unmixed, unburnt. McMillan (4), doing a student experiment in a new accelerator at the Kellogg Laboratory in 1933, wrote that the Li isotope ratio in the Sun is important. Richardson and I (5) followed that up in 1951 but found $^6Li/^7Li$ not reliable, even near sunspots. (Later, differing ratios of $^6Li/^7Li$ were found in young stars where a complex of nonprimordial origins exists.) Spallation by cosmic rays occurs in the interstellar cloud from which the star formed, with products modified by circulation and thermonuclear burning. The reduction in content from the Earth to the Sun is by a factor of 100. An experiment at Lick found Li on T Tauri stars, newborn from such gas, led Bonsack and myself (6) to find they had 100-fold increases. Beryllium (7) is less sensitive to hydrogen burning, which makes it a probe of deeper internal circulation. But what perversity of atomic structure placed the resonance lines of both neutral and ionized Be near the ozone cutoff of stellar spectra, 3300 and 3100 Å? Although that is a blow, boron is worse—with lines inaccessible at 2500 and 2100 Å. When I complained about this in a talk at Princeton in 1954, Henry Norris Russell reassured me that someday I would be able to see the B lines—clearly prophesying current space-based observations. The B lines have now been found in the oldest, metal-poor stars where it may be primordial; ratios of Li/Be/B are now being measured to determine their primordial composition.

I could not resist the challenge of the $^2H/H$ ratio; nature was again unkind, separating $^2H(\alpha)$ from the broad lines of $H(\alpha)$ by only 2 Å and setting their abundance ratio near 10^{-4}. At the Palomar coudé spectrograph I overexposed on emission nebulae for many hours but found that the scattered light was too strong. The isotope ratio $^3He/^4He$ was also tempting, with the solar chromosphere providing a brighter source; the ^3He isotope shifts vary from line to line, giving fine discrimination. But I found (8) no evidence for ^3He, to a quite low limit. Charlie Lauritsen took only 10 minutes to demolish the significance of that negative experiment, pointing out how easily ^3He self-destructs, critical in the termination of the *p–p* reaction and the solar neutrino problem. As an example of complication, Sargent and Jugaku (9) found that ^3He is more abundant than ^4He at the surface of a young star, 3 Cen A. If someone here wishes to use potassium as a new indicator, I warn that Nature, with its customary perversity, put KI resonance lines in the heart of the atmospheric O_2 absorption band at 7600 Å. Similar hard luck stories abound in spectroscopy and probably in all fields. It would take too long to explain later ramifications, but I hope to assure you that primordial abundances are becoming known and do agree with standard Big Bang parameters.

In 1950 no theory called for or predicted these light element abundances; if it had, the experiments demanded might not then have been possible. Many are still interesting. Dave Schramm tells me that an attempted explanation of abundances of the light elements by Fowler, Greenstein, and Hoyle (10) (using spallation in the solar system) did slow acceptance of the idea that useful primordial abundances are obtainable.

Perhaps we were stupid in the past, perhaps Nature is ungenerous. The cosmological enterprise will face, but I am sure surpass, similar roadblocks. It must succeed in making sense of the larger universe, which is, after all, not much more complicated than a star. Good luck to your enterprise.

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